

by Donahoe et al. is particularly sophisticated with its Hebbian-type rules and diffuse feedback arrangements. (Without perusing the Appendix in Donahoe, Burgos, & Palmer, 1993, I found the model difficult to understand as presented in Donahoe & Palmer, 1994, and in Donahoe et al.) Aside from issues of physiological relevance (which the authors have certainly tried to address), my concerns have to do with the properties of these models themselves. I count some nine free parameters in the present model, not including the constraints on the ranges of weights that determine the stochastic attractor states. How are the parameter values chosen? With so many parameters, why could not any data be simulated? Only simulated data were presented, so it is difficult to judge how well the simulation works except in the most general way.

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#### UNITS OF ANALYSIS AND THE ENVIRONMENTAL CONTROL OF BEHAVIOR

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We will begin our commentary on Donahoe, Palmer, and Burgos' article with some

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history that bears directly on our perspective on the current debate. From its outset, the research program at the Shriver Center and before that at the Massachusetts General Hospital had as its primary mission to understand and help to ameliorate problems of individ-

uals with developmental disabilities (e.g., Sidman & Stoddard, 1966). (The current authors received their training in this program and continue to pursue its agenda.) Dealing with practical problems of people with disabilities dictated research that would support more effective teaching. In particular, the understanding of stimulus control processes at that time was judged to be inadequate to the task (Sidman, 1976). An ambitious program was initiated, therefore, that featured the experimental analysis of stimulus control processes not only in persons with disabilities but also in other primates. That program led directly to the current torrent of research studies on stimulus equivalence and related phenomena (Sidman, 1994). Other aspects of the early work are less well known, although they may ultimately prove to be comparably important for both theory and practice. Because that work pertains directly to the matter under discussion here, we shall briefly review a few key findings and concepts.

Among the most important contributions of the program was research that demonstrated a unitary quality of environment-behavior relations. In work with rhesus monkeys, for example, Ray (1969) arranged individual stimulus elements with previously established discriminative functions in compounds that either preserved the original contingencies or reversed them for one of two elements. Subsequent tests revealed that exposure to so-called "conflict compounds" often did not alter the discriminative functions of previously established elements. Rather, the effect was merely to reduce the frequency of previously established environment-behavior relations in the conflict situations. When the original single-element conditions were reinstated, stimulus control by the elements was typically also reinstated immediately. These and related findings (e.g., Cohen, 1969; Stoddard & Sidman, 1971) supported the notion that environment-behavior relations were units, whose frequency of occurrence was related to the prevailing schedule of reinforcement.

Back then, the notion that reinforcement selected controlling environment-behavior relations was somewhat controversial and could easily have occasioned commentary in the spirit of Richard Shull's (1995) review of *Learning and Complex Behavior* (Donahoe & Palmer, 1994). In a precursor to the current

debate, Ray and Sidman (1970) reviewed findings from their own as well as other laboratories to emphasize the point that the analysis of stimulus control need not be restricted to the exteroceptive stimuli arranged by the experimenter. They argued that variations in the organism's own behavior could be an important source of stimulus control, even when the experimenter tried to hold other environmental factors constant. Donahoe et al. make a very similar argument in the section entitled "Interpreting behavior in nondiscrimination procedures" (p. 204). To our reading, Ray and Sidman's basic argument parallels in many respects those in the target article and in Donahoe and Palmer (1994).

In putting forth these arguments, Sidman and his group were not embracing the S-R tradition of Hull or Spence. In particular, they took a dim view of the intervening variables that were so prominently featured in S-R theories (e.g., see Ray & Sidman, 1970). Nevertheless, in their thinking and writing about stimulus control, they did depart from the mainstream behavior-analytic thinking and writing of some 25 years ago. For example, Ray (1969) and Stoddard and Sidman (1971) were unique in their use of the term *stimulus control topography* to refer to variations in controlling relations between environment and behavior. They also introduced the term *stimulus shaping* to refer to gradual changes in the stimulus control requirements of a task. Although there were direct precursors in Skinner's writings (e.g., Skinner, 1966, p. 18), these terms and the thinking behind them were not adopted by other behavior analysts. For the present authors, however, such concepts remain central to our thinking about stimulus control (e.g., Dube & McIlvane, 1996; McIlvane & Dube, 1992). In Donahoe and Palmer (1994), we found the thinking of kindred spirits who, although following a very different path from our group, have come upon similar basic conclusions.

By beginning the title of their article "The S-R issue," however, we think that Donahoe et al. may have made a small tactical misstep. For too long, behavior analysts have been mischaracterized by others as S-R psychologists. We fear that a highly visible debate of the "S-R issue" in *JEAB*, whatever its value, may be misunderstood by the naive reader.

Like Sidman's group, Donahoe et al. have never advocated a return to S-R psychology. We think that their article does a good job of showing that their positions are consonant with long-standing thinking in our field. For example, as students, most of us learned that Skinner believed that every behavior might well have an antecedent event, but that the technical challenges of identifying those events were daunting. The science of behavior could proceed, Skinner argued, by selecting appropriate analytical units (e.g., Skinner, 1957). Sidman (1986) has emphasized the need for flexibility in selecting analytical units—restricting or elaborating them to deal with the scientific problem at hand (see Sidman, 1994, and Stromer, McIlvane, & Serna, 1993, for a discussion of different ways to conceptualize these units). In this light, it might be more appropriate and have certain advantages to recast the current debate as one about appropriate “units of analysis” or “levels of analysis” rather than the “S-R issue.”

Donahoe et al.'s Figure 2, which shows a minimal selection network for operant conditioning, reminded us of Skinner's (1989) comment that

There are two unavoidable gaps in any behavioral account: one between the stimulating action of the environment and the response of the organism and one between consequences and the resulting change in behavior. Only brain science can fill those gaps. In doing so, it completes the account; it does not give a different account of the same thing. (p. 18)

Here, Skinner implicitly suggested a sort of a tag team, where the behavior analyst and neuroscientist would take turns wrestling with the stream of events that occur during operant behavior. On first glance, the upper portion of the network shown in Figure 2 seems to fit snugly within Skinner's first gap, and the VTA system originating in the lower portion of the figure fills the second gap. One question for Donahoe et al. is how they would characterize the relation between biologically plausible network models and Skinner's gaps.

On the one hand, if the activity of such networks is properly seen as “within” the gaps, then another type of network seems to be required to account for certain private events and covert behavior. Primary sensory inputs (S1, S2, etc.) may be located within the skin

so that such models can handle private stimulation like a toothache or feelings of hunger. But what about private events of the sort that Skinner (1953) referred to as “one's own discriminative behavior,” especially in cases of “a discriminative response that can be made when the appropriate [public] stimulus is absent” (p. 272)? For example, hearing an old song may set the occasion for remembering (“operant seeing”) a face from the past, which in turn may be discriminative for more private operant seeing and hearing. Can such behavior, which Skinner believed resulted from operant conditioning, be modeled by neural networks? To exclude it would seem to bring one back towards methodological behaviorism.

On the other hand, could the activity of one network constitute stimulation for another? To give the events that occur within networks the status of *antecedents* to behavior seems to raise other problems. For many behavior analysts, we suspect, neural antecedents will be judged as a departure from the standards of our discipline. Yet, we freely confess that we are struggling with how to come to terms with the findings of cognitive neuroscience without beginning to talk in this way (cf. McIlvane, Dube, & Callahan, 1996). Following Skinner (1953, 1974), we do not see how such private events differ in principle from more public ones, and we see no problem for behavior analysis when they are treated as intermediate links of behavioral chains (Flora & Kestner, 1995). We suspect that Donahoe et al. would agree that detecting them presents a challenge mainly for technology rather than theory.

Readers of this journal may question an apparent inconsistency in this position and our recent response (McIlvane & Dube, 1996) to Horne and Lowe (1996), in which we objected forcefully to covert “naming” as the fundamental mediating event underlying stimulus equivalence. We feared that Horne and Lowe were sliding down the slippery slope towards naive cognitive psychology. But no such fears follow a reading of Donahoe et al.'s essay. They clearly understand the difference between direct experimental analysis of biological processes (or models of such processes) and making guesses about purely mental events and structures. The latter enterprise, of course, has been a source of vexation for

Skinner and the generations of behavior analysts that followed him. Enterprises of the former type, however, are all too infrequent; increasing that frequency, we believe, will ultimately lead to scientific contributions that behavior analysts are uniquely able to make.

If we have any quibble with Donahoe et al., it is with their degree of emphasis on neural network modeling techniques. Although we are greatly impressed by their work (particularly that which simulated three-term contingencies), it would be unfortunate if the readers' take-away message was the statements on page 193: "complex human behavior *typically* occurs under circumstances that preclude experimental analysis" (emphasis added) and "the most compelling interpretations [of such behavior] promise to be those that trace the cumulative effects of reinforcement through formal techniques, such as adaptive neural networks." Although we are sympathetic to the circumstances that occasioned their remarks, we have more faith in direct experimental analysis of complex human behavior. Also, although we agree that simulation techniques may have much value, the intended use here seems to rest not only on the biological plausibility of the model but, more importantly, on its biological adequacy (i.e., the concordance between the processes modeled on computer and actual biological processes). We are not in a strong position to evaluate biological adequacy, and we think that Donahoe et al.'s arguments would be strengthened by greater attention to this issue.

There may be yet another danger in placing too much emphasis on simulation techniques, particularly in the absence of compelling biological adequacy. We refer to the "flight to the laboratory" available to those who judge the world outside the laboratory to be too difficult to manage scientifically. Given the nature of our disabilities research program, we confront such difficulties on a regular basis. We also know, however, that earlier simulation efforts in the disabilities field (e.g., the stat-children of Zeaman & House, 1979) have not had much lasting value, whereas direct experimental analyses like Sidman's early equivalence experiments continue to influence researchers more than 25 years later. When embarking on an effort to simulate human behavior, therefore, we think

it wise to remain as closely connected as possible to experimental work with humans, integrating where possible the efforts of behavior analysts and neuroscientists. With the advances in online neuroimaging techniques one may reasonably anticipate over the next few decades, the activities of biological neural networks (i.e., living brains) seem likely to become increasingly available for analysis. For those who seek to establish active biobehavioral partnerships, simulations like those of Donahoe et al. may provide useful adjuncts with considerable heuristic value.

To conclude, we think it appropriate to point out one similarity between the two articles for which *JEAB*'s editors have recently invited comment. Is it just a coincidence that both articles have been relevant to covert processes that might modify relations between environmental events and behavioral ones? We think not. Like our colleagues in other branches of behavioral science, we behavior analysts are interested in as complete an understanding of behavior as empirical and theoretical science can give us. There may be growing understanding within our field that completeness will require explicit efforts to understand processes within the skin (cf. Rilling, 1992). We applaud Donahoe et al. for their efforts to help our field come to appreciate what is likely to be an important part of its future.

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### THE ROLE OF MOTIVATION IN THE S-R ISSUE

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Donahoe, Palmer, and Burgos have attempted to resolve the seeming conflict between two different ways of interpreting the effects of reinforcement. Donahoe and Palmer (1994, p. 68) state that “what is selected is always an environment–behavior relation, never a response alone”; and in his review of their text, Shull (1995, p. 354) questions whether this view is consistent with the be-

havior-analytic view that “operant behavior occurs in a stimulus context, but there is often no identifiable stimulus change that precedes each occurrence of the response.” The problem with this formulation is that Donahoe et al. imply that the only environmental variable that can be a part of an environment–behavior relation that can be selected by reinforcement is a stimulus that is immediately antecedent to the response. Shull accepts this limited view of the environment and questions its adequacy for dealing with those circumstances in which there is a constant stimulus context and yet the frequency

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